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Basic Concepts of Synoptic Analysis

(Representing a Book Review and a Criticism of that Review)

S. P. Khromov (reviewer) and G. D. Zubyan (critic).

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REVIEW OF BASIC CONCEPTS OF SYNOPTIC ANALYSIS

S. P. Khromov

[Note: The following is a book review that appeared in Meteorologiya i
Gidrologiya, No 2 (October 1950), pages 20-26. A critical answer to this review,
by D. D. Zubyan, appeared in the same issue, pages 27-33.]

(In connection with the work by Kh. P. Pogosyan and N. L. Taborovskiy
[deceased]: "Advective-Dynamical Bases of Frontological Analysis". Trudy
TsIP, No 7 (34), 1948. [Trudy TsIP means "Works of the Central Forecasting
Institute".])

In the reviewed work the authors are making the first steps toward a survey
of relations between the frontological conceptions and the notions of "advective-
dynamical analysis" developed by them. The size of the work (77 pp) compels
us to pause only on its basic statements, important for practical analysis.
Therefore concepts of individual and local frontogenesis remained undiscussed.

1. Geographical Classification of Aerial Masses

The geographical classification of aerial masses really has no direct
prognostic value. Moreover, the synopticians, contrary to the statements by the
authors, never relied in their forecasting on average characteristics of aerial
masses, if we do not take into account the suggestions by Schintze, Meze and
Rossby on use of homologues, which were of little effect on Soviet practice.
Nevertheless geographical classification is rather essential for analysis,
and therefore indirectly for forecasting. The geographical type of mass,

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correctly determined, supplies us with a natural complex of weather conditions, within the range of condensed characteristics of its system (for a specified time and location). Every experienced synoptician presents well the specification and peculiarity of the weather, let us say in the summer KTV in the central territory of European USSR or the winter MPV in the northwest etc. Further on, the geographical classification of aerial masses considerably facilitates concrete analysis of synoptic situation, not by applying average characteristics, but by explaining the role of masses and fronts in the general circulation mechanism in the specified synoptic situation. The important value of geographical classification and of average characteristics of masses for climatology unequivocal; another value of synoptics is that from its materials climatologists will make their conclusions. Therefore it is reasonable, despite their full denial, to consider geographical classification of aerial masses; the authors nevertheless recommend that indications of basic geographical types be left on synoptical maps. Here definite progress may be found in the views of the authors: back in 1941 they did not "acknowledge" aerial masses.

Although in Russian the word combination "umerennyy vozdukh" (temperate air) sounds unusual, it seems nevertheless the best variant among possibilities, and its introduction eliminates conditionality and misunderstandings in terminology.

2. Geographical Classification of Fronts

In this connection it is however appropriate to introduce a new equivalent to the term "polar front". The geographical classification of fronts inevitably results from the classification of aerial masses, and the attempt of the authors to deviate it can in no way be convincing.

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The authors pretend that "concepts of arctic and polar fronts, taken as basis of any analysis of a synoptic map, as a rule lead to difficulties, which could be avoided only by the formalization of synoptic analysis". We are of different opinion: this concept imparted to analyses of our synopticians a coherence and depth in connection with the general circulation, lacking in, e.g., German analyses. A denial of this concept would threaten nihilistic chaos instead of analysis. The difficulties mentioned by the authors really did sometimes happen. But the reason for their appearance is just the formal non-dialectic approach to synoptic analysis, stopping confusedly, e.g., before the fact of conversion of the arctic front into the polar one, or before the simultaneous presence on the map of two arctic fronts in the same natural synoptic region, or before a cyclogenesis on an occlusion front. Every concept can be led to absurdity and may be formalized in its mechanical application, but it may also be rendered valuable if used as its dialectical subject, reflecting its dialectic nature, requires it.

In developing their arguments, the authors indicate that "the main front often appeared virtually in the zone of small contrasts of temperature, while the secondary was located in the zone of big contrasts". Some unexperienced synopticians could see it that way, but this would only indicate an incorrect and shallow analysis. In a correct case the defined main front essentially (as far as it separates aerial masses of different latitudinal zones) cannot contain the greatest contrasts, if, of course, we take into account representative temperatures. Maps of relative topography may be here of essential help, by clarifying the average temperature distribution in the lower half of the troposphere; and the more they will be applied in the determination of the

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front type, the better. It is of course possible that in the beginning of the formation process of the main front or during its degradation temperature differences of minor importance may appear, but for somebody used to dialectic thinking this circumstance is not disturbing.

We agree with the authors that although not all, but many secondary fronts were of arbitrary relation to occlusion fronts without connection with the previous front history. But no basic difficulties are involved here, only insufficient experience and carefulness in analysis. The concept of secondary (of second grade, according the terminology of the author) fronts was excluded by nobody from synoptics.

3. The Front and The Essentials of Synoptic Analysis

The authors evidently belittle the role of front for the comprehension of synoptic processes, stating that the analysis by means of baric topography "showed that we may discuss the development of processes on basis of analysis of thermobaric fields, without taking fronts directly into account", their effect on dynamics of processes being expressed "totally by horizontal temperature gradients and pressures in the troposphere". Let us remember there was a time when synoptic analysis was satisfied only by baric relief at sea level and the circulation and first of all the cyclogenetic effect of the then yet unknown fronts was accounted for vaguely and "in general". It is worth while to retrograde synoptics back to this period at a time when the reality of tropospheric fronts does not arouse anybody's doubts, including the authors? At the present time it is necessary to tend not to generalization, but to synthesis of such concepts, which are real, practically tested and doubtlessly possessing physical connection. The separation of baric topography maps from real frontal surfaces, not observed as such on maps of relative topography

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(owing to their smoothing and averaging properties) is by itself not the most important or most strong phase of advective-dynamical analysis. Obviously even the authors understood it, because while stating the possibility of dealing without fronts, they nevertheless are preoccupied with fronts in their work.

The connection of cloud systems and precipitations just with frontal surfaces, and not with blurred high altitude frontal zones on maps of relative topography (those being only peculiar reflections of these surfaces) is not denied even by the authors. But it is incomprehensible why they think that "the Norwegian school considered it rational ... to place foremost the kinematic characteristics of processes and to leave in a secondary role the dynamic characteristics." The works of Norwegian meteorologists cannot lead to such a conclusion. But if by "Norwegian" school we shall understand a frontological school in general (the achievements of which the authors try to undermine in their other statements, ascribing them to mythical "Norwegians" or proving that no results were attained since 1930) it is also impossible to come to such a conclusion.

The absence of a strict quantitative account of the dynamics of the processes was not an adjustment, but a sad necessity for frontological synoptics as well as for advective-dynamical analysis, until the system of Kibel appeared and developed into a synoptic theory by N. L. Taborovskiy only in 1947. Frontology has been in use since long ago, although only qualitatively, a series of dynamical concepts; let us remind ourselves of the theorem of acceleration of circulation, the theory of surface separation, the theory of undulatory cyclogenesis etc. These theoretical situations served as rational bases of one or other methods of practical analysis. At the same time the advective-dynamical analysis rested until 1947 on concepts of advection and convergence of isohyps, which in no way revealed the dynamical mechanism of the phenomena.

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It is not the first time that the subject of frontological analysis has been characterized by the authors as being against objective reality, i.e. against frontological methods. The authors attribute to frontological analysis all that they consider necessary. So in 1947 Kh. P. Pogosyan, N. L. Taborovskiy and K. I. Kashin (cf: Meteorologiya i Gidrologiya No 6 (1947) considered that "the basic principle of this (frontological - S. Khramov) method is the distinction of aerial masses according to their conservative features, among which the most important is the equivalent potential temperature". Meanwhile every practicing synoptician understands that the basis of the frontological method is the exploitation of fronts and the frontal interpretation of cyclogenesis. This just leads to the dismembering of the troposphere into aerial masses, defined first of all by their circulation role and not by statistical characteristics computed in advance. New matter which frontological analysis introduced into the practice of forecasting is derived first of all from the physical concept of front, its evolution and displacement; secondly from the physical concept of frontal cyclogenesis with their effect on structures and dislocations of disturbances; third, from concepts of isentropic and particularly entropic reactions on aerial masses. The introduction into synoptics of a third dimension may really give fruitful results, namely on basis of such solid frontological concepts as front and frontal cyclogenesis; we should enjoy it that, judging by the work of N. A. Taborovskiy in 1947 and by its review, the authors understood it. A three dimensional synoptic would not really be a fully evaluated two dimensional synoptic, if the matter were limited only by altitude vergence and advection or by altitudinal deformational fields, on which the advective-dynamical analysis of 1947 was constructed.

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4. Cold and Warm Masses

We find further a suggestion that aerial masses be subdivided into cold and warm ones according to distinctions found among them on maps of relative topography. This suggestion is quite natural: temperature contrasts among aerial masses of course are more reliably defined by relative topography, than by an insufficient picture of the temperature field near the ground.

5. Transformation of Aerial Masses

Nothing new is to be found in the authors' treatise on the problem of the transformation of aerial masses; but the problem of transformation in active centers (of isolated masses, according to the terminology of the authors) is set upside down. The authors positively deny the presence of tropical air in the altitude of upper cyclones, and therefore conclude that in high-altitude cyclones the cold air is transformed into a warm one, but not the contrary; the opposite effect is observed in high-altitude anti-cyclones. A virtual confirmation of this supposed absence of warm air at altitudes "over the center of occluded (cold) cyclone" is not given in the article. However it seems to us that this absence or presence is impossible to notice on the map OT 500/1000. In the lower five kilometers the central cyclone is certainly filled by cold air; but this cannot prevent the tropical air from extending itself over the cold one in the upper part of the troposphere and undergoing transformation just there, as it was assumed until now. At the same time we cannot leave unnoticed the effect of cold intrusions into low latitudes with final anti-cyclones.

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By making the synoptic mechanism of the transformation precise, we could say that transformation PV and TV originates in the conversion process of low (at first) final anticyclone of the series into a warm and high subtropical anticyclone. Therefore on altitude charts the transformation will proceed in the high altitude (cold) cavity and will end together with the liquidation of this cavity. Similarly the transformation TB and PV is connected with the liquidation of the warm crest over the occluding cyclone, i.e. with the process of conversion of the low and medium cyclone into a high-altitude one.

It is doubtful that vertical motion has a secondary (as thought by the authors) role in the transformation. In an anticyclone just downward motions will effect temperature rise in the lower part of troposphere; and radiative decrease, mentioned by the authors as the cause of transformation of warm air into cold one, is limited only to the layer near ground, and moreover only during the cold season.

The further subdivision, introduced by the authors, of masses into marine and continental is certainly not new and not worth discussing.

6. "Qualification" of Fronts

Further on the authors introduce a distinction between tropospheric and near-the-ground frontogenesis. Such terminology is not very successful, as far as the friction layer concern also the troposphere. The authors connect "tropospheric" frontogenesis only with deforming fields; however convergence as a front forming factor can hardly be excluded from analysis - not the convergence of friction, but the convergence of the great vertical propagation as a factor, if not of basic importance (as in tropics), at least accompanying deformations. "Near-ground" frontogenesis, connected with the convergence of friction, according

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to the authors, "is rather closely connected with tropospheric frontogenesis" and therefore should be related to the increasing (in the case of frontolysis, blurring) effect of friction vergence on the lower parts of "tropospheric" fronts. The same "near-ground" frontogenesis, according to the authors, is able to create also fronts "of very small vertical extent, not connected with frontal zones of the troposphere ... e.g., secondary fronts". This also is not new: the synopticians never thought of connections of secondary cold fronts with basis frontogenetic fields of the troposphere (i.e. with deforming fields) but ascribed them to the increase of internal mass gradients of temperature under effect of convergence. It is obvious that the appearance in this way of secondary warm fronts is less probable because of more homogeneity of warm masses, but cannot be excluded by principle.

Therefore we see a sufficient analogy between the main fronts and tropospheric fronts of the authors, and between the secondary fronts and the ground level fronts of the authors.

It is true that, as is clarified later, the authors do not name all tropospheric fronts as basis (i.e. active in relation to cyclogenesis) but only those in which temperature contrast in the frontal zone attains or exceeds 16 db per 1000 km. The remaining tropospheric fronts together with near-ground ones are called secondary by the authors. We cannot say anything against the division of fronts into cyclogenetically active and cyclogenetically inactive. But the authors formalize this subdivision on the basis of some fixed magnitude of temperature contrast, which allegedly is a transition point from inactivity to activity. This "constant", however, fluctuates in various works of the authors in rather wide limits, from 16 to 24 db.

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It is rather natural to assume that in reality it fluctuates within still wider limits, because it is not the only parameter defining the process of cyclogenesis.

One way or another, we could conserve without harm to the main subject the term of main fronts instead of tropospheric fronts, as by the authors, and to speak of active and inactive main fronts, instead of basic and secondary.

The authors could reply that their basic front does not necessarily separate aerial masses of basic geographical types, e.g. arctic and polar air, but is able to separate marine polar air from continental polar. We think however that the thermal contrast will be at its peak in the case of main fronts and therefore will secure for them the "qualification" of basic. Connected with them will be cyclogenesis, as shown by experience, while each main front in non-tropical latitudes is cyclogenetic until its peak. But if, e.g., cyclogenesis starts on the occlusion front, then the occlusion front transforms during that period into a main front.

7. Occlusion Fronts

The authors' concepts in the matter, that a cold occlusion represents, so to speak, a normal case with respect to altitudinal thermobaric field, are justified. It does not mean that warm occlusions will be a seldom phenomenon, at least in Europe during the cold season. But it means that in such cases the vertical power of the frontal cold mass should be sufficiently small, as not to be noticed on maps OT 500/1000. This was often observed in Europe and in America. Probably it is sometimes difficult to distinguish this type of occlusion from a front covered by a film of cold air above the continent.

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In general the conclusion by the authors of frontolysis of the internal part of the occluded cyclone corresponds to experimental data. Every synoptician knows that often the search of fronts near the center of the occluding cyclone is a difficult operation, leaving a wide range to arbitrary judgement. The tendency not to part with the once found front leads to great tensions.

The authors are also correct in that occlusion fronts are often abused by synopticians, particularly in considering secondary cold fronts as bent parts of occlusion fronts. But experienced synopticians always considered such long "tails" of occlusion fronts as an elementary error. The length of the tail in any case should not exceed the distance covered by the cyclone center after the beginning of the occlusion, and usually it is not great. The inevitability of blurred fronts in an occluded cyclone was always acknowledged, which evidently limits the frequency of occlusions.

It is not clear, however, why the authors deny by principle the possibility of a curved occlusion independent of frontolysis. The authors say "occlusion fronts do not expand inside a cold region; therefore occlusion fronts cannot, in accordance with structure of altitude currents in a cyclone, pass by bending into the rear part through the center of cold (p. 52)." However it seems to us that the "transfer through the center of cold" is not obligatory. For a bending of the occlusion it is quite sufficient that the warm altitude crest, turning in the cyclonic direction, find itself northward from the south end of the cold cavity, as shown by the authors on Figure 25c. Then the bent part of the occlusion front may pass in such a way that the altitudinal seat of cold will locate itself in its rear; and it is not understandable why the occlusion front could not be continued this way on the figure. It would be impossible only in the case of full coincidence of the minimum on map OT 500/1000 with

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the near-ground baric minimum, i.e. in the case of liquidation of the warm crest in the internal region of the occluded depression. But this will occur only in the case where the warm air in the occlusion system will be transported above surface on 500 mb or, at least, so far as its presence will no longer noticeably affect the distribution of average temperature in a layer from 1000 to 500 mb.

It is rather probable that the temperature distribution on isobaric surfaces or, at least, relative topography of thin layers is more able to shed light on variations of the temperature field during occlusion than the maps OT 500/1000.

8. Fronts in the Anticyclone

It is doubtful that inversions in the free atmosphere of the anticyclones should be connected with frontal surfaces, forming the boundaries of two basic aerial masses (as may be concluded from p. 57). In an anticyclone several inversions are rather frequently found, located one above the other. If one of them is frontal (in the sense of the main front), the other should be related to phenomena of internal mass deposits, similar in some way to secondary fronts. We cannot agree with the statement by the authors on the "rather secondary" role of the vertical motions of the air in the process of formation of inversions in anticyclones. It is sufficient to remind ourselves of frequent frequent temperature jumps on frontal surfaces in anticyclones up to 20°, which cannot be explained by horizontal displacement, and also the drop of relative humidity in the inversion layer, clearly connected with temperature increase with conservation of potential temperature and specific humidity in the individual element of air.

In summary we may say that the connections between the aerial mass and the front on one side and the subjects of the advective dynamic analysis on the other side are not sufficiently outlined in the specified work and further

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investigations along this line are desirable. Only in this way will it be possible to attain a synthesis of the frontological method and the advective dynamical analysis in practical work, a synthesis which until now is still considerably substituted by mechanical combination. At the same time some situations in the work are not analyzed enough, some are not new subjects, and there are no reasons to introduce radical changes or principles into the techniques of the frontological analysis of near-ground maps. Separate conclusions and suggestions by the authors (e.g. distinction of warm and cold masses on topography maps, criticism of some suggestions concerning occlusion, etc.) are, of course, correct and are worth consideration in practical work.

The discussed work does not belong to fundamentals of advective dynamical analysis, and our criticism does not concern these fundamentals. We are of the opinion that advective dynamical analysis has indisputable advantages and value within its range. This is the range of synoptic connections governing the variations of the baric field. But within the range dealing with fronts and aerial masses the authors did not succeed in attaining equally valuable results.

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